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KENNETH G. SCOTT:  
RADIOISOTOPE RESEARCH IN MEDICINE

An Interview Conducted by Sally Smith Hughes

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Kenneth G. Scott

ca. 1947





Kenneth G. Scott and William A. Reilly  
with the staff of the Radioisotope Laboratory  
Veterans Administration Hospital, San Francisco, 1953



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## **Acknowledgment**

This interview with Dr. Kenneth G. Scott is one of several dealing with the development of Crocker and Donner laboratories, within the larger series of oral histories produced by the History of Science and Technology Program.

Besides these interviews, the Program assembles other primary source materials, including the papers and personal memorabilia of scientists and engineers, and the papers of certain organizations with which they were associated. The papers and interviews help to document the development of science and technology not only in the western United States, but also in the nation as a whole.

The work of the Program was made possible initially by the generosity of William R. Hewlett and David Packard. Mrs. Calvin K. Townsend established the Doreen and Calvin K. Townsend fund to provide ongoing support of the Program. The University Endowment Fund, National Science Foundation, and National Endowment for the Humanities have assisted diverse aspects of the Program with a series of grants. Further aid has come from the Marco Francis Hellman Fund, established to document science and technology and their relations to business in California. Other donors have included the Woodheath Foundation, the California Alumni Foundation, and the Watkins-Johnson Company.

James D. Hart  
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## Introduction

This transcript is based on an interview with Kenneth G. Scott on December 17, 1979, at his home in Novato, California. Dr. Scott, who was diabetic and had been partially disabled by a stroke for several years, was interviewed at bedside. The interview, conducted by Sally Smith Hughes, deals with Dr. Scott's research on radioactivity in medicine and his long association with Crocker Laboratory and the University of California Medical Center.

This interview is part of a series recorded during 1979-1980 with individuals associated with Donner and Crocker laboratories and, in some cases, the Division of Medical Physics and Biophysics. Donner Laboratory is part of the Biomedical Division of what since 1971 has been called the Lawrence Berkeley Laboratory; Crocker Laboratory no longer exists. The Division of Medical Physics, which in March 1980 became a department, is an academic unit of the University of California, Berkeley.

This medical physics interview series, conducted under the auspices of the History of Science and Technology Program at The Bancroft Library, is part of a project funded by the National Endowment for the Humanities to document the history of the Donner and Crocker laboratories, the Division of Medical Physics and Biophysics, and the careers and research of scientists and physicians associated with these institutions. These interviews, in conjunction with archival holdings at The Bancroft Library and Lawrence Berkeley Laboratory, illustrate the growth of the fields of medical physics and biophysics, in which Berkeley research and academic institutions played an early and significant role. They also complement other interviews and documents in Bancroft Library concerning the development of Lawrence Berkeley Laboratory.

Oral history can frequently provide information on subjects not easily retrieved from published sources. These subjects include family history, social and economic conditions affecting research, interactions with colleagues, peculiarities of institutional organization, and perceptions of a scientific discipline. It should be borne in mind that the interview records what the subject remembers during the interview about



what happened at a given place and time; typically, many years have passed since the events occurred. In general, information obtained in response to broader questions is more likely to be accurate than answers concerning specific events, influences, and accomplishments.

The verbatim transcript of this interview was checked against the tape recordings and edited for punctuation, paragraphing, spelling, verification of proper names, and repetition. Because interruptions for nursing care affected the flow of the interview, several sections were rearranged for better topical or chronological sequence. Dr. Scott read and approved the edited transcript but made no corrections or additions apart from answering specific questions concerning proper names and, in a few cases, interpretations. Jennifer Snodgrass aided in the final production of the transcript; the index was prepared by Robin Rider.

Literary rights for this interview are vested in the Director of The Bancroft Library. Any quotation for publication of the material included herein requires the advance written approval of the Director. A request to see the transcript constitutes an agreement to abide by this restriction.

Background materials pertaining to this and other interviews in the History of Science and Technology series, together with tape recordings of interview sessions, are available for research use in the Heller Reading Room.

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Robin E. Rider  
History of Science and  
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**Curriculum Vitae**  
**Kenneth G. Scott**

1909	Douglas, Arizona, born February 6 father: Lee Charles Scott mother: Anna Jane Harris Scott
1934	University of California, Berkeley, A.B.
1934-37	Research Assistant, University of California, Berkeley
1937-39	Research Assistant, Crocker Laboratory, UCB
1939-42	Businessman
1942-51	Research Associate, Crocker Laboratory, UCB
1946	Civilian Scientist, Operation Crossroads, Bikini
1946-48	Consultant, U.S. Navy
1946-48	Consultant, Scripps Institute of Oceanography
1948	University of California, Berkeley, Ph.D. (physiology and biophysics)
1948-51	Assistant Professor, University of California, San Francisco
1948-49	Consultant, U.S. Atomic Energy Commission
1948-50	Director, Radiobiology Laboratory, Laguna Honda Home
1948-	Consultant, Veterans Administration Hospital, San Francisco
1951-	Associate Professor and Director of the Radioactivity Research Center, University of California, San Francisco
1951-	Member, Advisory Board to Governor's Committee for Radiological Safety



Date of Interview: 17 December 1979

### **Family Background and Early Education**

Hughes: Dr. Scott, would you tell me a little bit about your family background; maybe start with your grandparents and tell me their names and where they came from?

Scott: Yes, I'm from Arizona originally, and my maternal grandparents [William and Jane Rose Harris] were immigrants, mostly from Wales. They settled in Bisbee, Arizona, and worked for the Phelps-Dodge copper mines. They're no longer open; it's just a ghost town there. I enjoyed my boyhood very much with them; I was around [them] till I was about six years old. They lived right on a street called Brewery Gulch, which was loaded with secondhand stores. They'd built the Saint George Hotel, which still stands on the land that they had. The original home was completely removed. It was burned out or something.

I was born in Douglas, Arizona, on February 16, 1909. And I had some schooling in New Mexico. My father [Lee Charles Scott] was operating a steam shovel. He was sort of a locomotive engineer. I went to the first grade in Santa Rita, New Mexico, and I found it a very interesting place. It was primarily Chicanos, or Mexicans, or whatever we used to call them—"greasers." From there we moved back to my original place of birth, Douglas, and I went through two grades there. Then my mother [Anna Jane Harris Scott] and father were continuously fighting and separating and getting back together again. I got a little fed up with the whole thing.

I have an older brother and a younger brother. My mother moved us to Los Angeles during the latter part of World War I, which I remember very well. She was taking a



beauty-school course, but she never did anything with her courses. She later inherited part of her mother's fortune, which came around in the Depression. She was back in beauty school, but she gradually frittered away everything. I left home at that point; I was going to school at UC Berkeley. I should mention that all of my [grade] school and high school was done at Dunsmuir, California, which was a big railroad [town].

Hughes: By that time had your mother and father split permanently?

Scott: They never did, except periodically.

Hughes: So your father was always around to support the family?

Scott: Well, you could call it that. He was quite a drinker, and he would disappear with all of the money that he had and stay away till it was gone.

Hughes: Did that make it pretty rough on your growing-up years?

Scott: Well, they were very limited as to what we could get. My answer to that was to go get a succession of jobs. Dunsmuir, as you may know, is near Shasta Springs. I first got summertime money by establishing a vegetable route. I'd go around and get orders for fresh vegetables and then deliver them in a baby buggy that I had. I raised a lot of rabbits and I would sell them from door to door and take orders in advance and then go prepare them. I liked that and I liked animals.

Hughes: Were you beginning to be interested in science in high school?



Scott: Not really. I was so busy working in high school that I didn't ever get to take a lot of courses. I took the required mathematics. And it wasn't much of a school we had in Dunsmuir; it was a community school district. But I learned the most and got the most out of people when I was employed by one of the local pharmacies, run and owned by Cecil Jones. He taught me more about dealing with people and general problems than anyone I can remember. I'm really indebted to him.

Hughes: Just by example?

Scott: Mostly by example. He was a father figure to me. I think I got paid \$70 a month. But I worked every afternoon, every Saturday, every other Sunday. So I didn't have the time to do a great deal [else]. I wasn't really a good sportsman anyway. I wasn't performing physically like I thought I should; so I gradually got away from that.

Hughes: Did he have any part in your going on to Berkeley?

Scott: No, he didn't. I got a better-paying job later on. I guess about half of my high-school career I worked for a haberdashery shop called the IXL [Men's Shop], which was run by Mr. Welsh, who later became postmaster of Dunsmuir—I think he's still around, in his eighties—and an old character by the name of Gerkey. They ran this haberdashery store and we sold really fine clothing. The shirts that Macy's wants \$20 for now, sold for \$3.50.

So I got through high school, and I spent another year preparing myself for the college of business administration—it was mostly economics at Berkeley.

Hughes: Was it your idea to go on to college?



Scott: Yes.

Hughes: Neither of your parents had college educations.

Scott: No.

Hughes: Did they care about your education?

Scott: Well, not really too much; my parents were really out of it. My father barely got through the third grade. His mother was a very severe Baptist type, whom my mother didn't like, and they had their battles up and down. They lived in New Mexico, and her husband, my father's father, died early, at the age of about 47. She married again, a retired engineer, and was quite concerned with us as children because she could see what was happening. I refused to go to Sunday school—didn't want to give them that nickel. When she died, her property was given to my father, all of it. There were some country homes and some very nice vacation places up in the mountains in New Mexico.

Hughes: Was that connected with the copper mines? Was that where the money came from?

Scott: No, she didn't have anything to do with the copper mines. The Phelps-Dodge company installed an ore roaster and a copper refiner in Douglas, so the good ore went down there on the train. It's a matter of 15, 20 miles.

So my days at Douglas were really uneventful. I went by there a few years ago and the place is still there. [It] had big white pillars like an old Southern plantation type



of place.

**University of California, Berkeley**

Hughes: Well, how did you get the idea of going on to college, since it doesn't sound as though you were in an environment where that kind of thing was very common?

Scott: Well, it was strictly non-academic. I had the desire to become a physician and surgeon, and I never really gave that up, even when I was in college. I changed my major several times. I used to bug my advisor, because I'd promise to do something and I'd come back the next year with a whole new program, and he couldn't remember from one year to the next. He was in the bacteriology department.

Hughes: But you originally had the idea of going into business?

Scott: Yes, I did.

Hughes: Why business rather than a pre-med course?

Scott: Well, most of the people that I admired were salesmen and people of that sort, who came by with 50 suitcases' worth of samples and stuff, and stimulated me into a business career. I took extra courses in mathematics at the high school as kind of a fifth-year student. Our principal there was a Mr. L. R. Switzer. The school board was down on him, and as students we held a march and an uprising against an old lady who was the head of the school board, and put the kibosh on that. When I did go to Cal he was very careful about my grades; he gave me excellent grades. I think he even changed the final grade I got.



Hughes: Well, was it unusual to send a student, a graduate from Dunsmuir, to Berkeley in those days?

Scott: It was, rather, yes.

Hughes: So you were sort of a prize student.

Scott: Well, not really. I was lousy in everything, in English, in history. We had a lot of unusual teachers there.

Hughes: But you were good in math.

Scott: Well, I was fairly good in advanced algebra.

Hughes: If you were so bad, as you say, in all of the other subjects, why was he encouraging you to go to Berkeley?

Scott: Past favors, I think.

Hughes: What happened when you got there? Was it rough for a while?

Scott: Well, I entered in the class of 1928. I had a cousin, who is retired now and still lives in Bisbee. I haven't had any contact with him for years. But he was in the economics department as a teaching fellow. His name is [William] Bruce Lockling. He got me enrolled, and this was a very confusing thing to me, because it was so much bigger even then than anything that I'd witnessed. I had a devil of a time. The first semester I just worked my tail off trying to get somewhere, but I always had that money problem too.

Hughes: Had you saved money from your jobs?



Scott: No, all I had when I arrived in Berkeley at UC was \$50, which paid my tuition.

Hughes: What did you do to supplement the \$50 to get you through the year?

Scott: Well, I got a room job, waiting on tables at a sorority. I don't remember—the years all flow into each other—but I had a lot of jobs. I used to roll down the course in a peewee golf course and water [it]. That lady there was very suspicious of me. She didn't think I did anything. Well, you water in Berkeley, and in a few minutes it's all dried out.

Hughes: How did you do as an undergraduate?

Scott: This begins my real scientific career. I took a course in physiology, and [when] I could devote all the time that was necessary, I did very well, exceedingly well.

Hughes: Who taught the course, do you remember?

Scott: Well, Sherburne Cook, whose name is scattered all through these [reprints].

Hughes: Was it the introductory physiology course?

Scott: Yes. We had Eric Ogden there and a fellow I've lost track of, by the name of Simpson—they were instructors in the department—and Milton Rose, who later took up psychiatry. I ran into him quite a few times when I was giving courses on nuclear medicine.

Hughes: These people were all instructors at that stage?



Scott: Yes. They used to play with each other, sending fake emergency calls in to them in the middle of the night, and getting them out and so forth. It was quite a nice place. This was at the old Spreckles building. Now I guess it's the art department on the campus. It was a beautiful red-wood building, probably designed by Maybeck. It housed the laboratory, which is very close to the Men's Faculty Club. I wasn't aware of its existence till much, much later, but that's where I came from.

Hughes: What was it about the course that caught your attention?

Scott: Well, it was very stimulating intellectually to me. And the place had a beautiful library and I used to satisfy my curiosity by going down there and reading books.

Hughes: Was it more physiology itself than the lecturing talents of Sherburne Cook? Or was it a combination?

Scott: Well, it was mostly the laboratory, which I found very good and interesting and stimulating. We did the usual physiological things, like the frog and the turtle and later on the dog and so on.

Hughes: Were you making any decisions at this stage about what you wanted to do later in life?

Scott: Well, I had a bulletin from every medical school in the United States. I was always looking at a course that would cut the academic time down to zero.

Hughes: Was that for economic reasons?

Scott: Yes.



Hughes: What year was this?

Scott: This was in the early 30s. This must have been '30 or '29; I kept hopping back and forth.

Hughes: You were a sophomore or a junior?

Scott: I was a sophomore.

Hughes: Was that course enough to switch your direction towards pre-med?

Scott: Well, I really took all the courses that were required, like zoology, and we suffered through the shark....

Hughes: Did you have any personal relationships with any of these people in physiology or in any of the other science courses you were taking?

Scott: Yes, I lived at Cook's home for some time, and they were always very nice to me. They had three sons who were young children at that point. I used to enjoy them. I eventually married Sherburne Cook's sister-in-law, Doreen Cox. We ran away to Los Angeles, which made him very angry.

Hughes: When was this, now—after you were in school?

Scott: This was right in the middle of the Depression. I dropped out of school, and I realized sooner or later I was just going to have to go back there and get through. Among the papers I have around here is my diploma. I got out of there in '36.



Hughes: Was the diploma in physiology?

Scott: Bachelor in physiology, yes.

Hughes: What were you thinking at that stage, when you finally got your bachelor's degree, about what your career would be?

Scott: I didn't even go to graduation; I was doing some experiment that I thought was important. I can't remember now what the deal was; I was in many fields.

Hughes: Were you doing this on your own time, without an official position?

Scott: Yes. They gave me any amount of space I needed and a little money, very little, about \$500 a year.

Hughes: This is what Cook did?

Scott: Yes.

Hughes: Why did he single you out, do you think?

Scott: I don't know.

Hughes: He must have known you were good.

Scott: I never really found out from him. He was a New Englander type and had very little to say.

Hughes: But it sounds as though he was encouraging you scientifically, anyway. It's unusual for an undergraduate to be given research space and a little bit of money.



Scott: Well, he did. He went away for one summer. His family was all on the East Coast somewhere—Hartford, I believe. He was very interested in termites. I wasn't terribly interested in termites, but I found out that no matter what I was working on, it became very interesting in the end. He told me to work out the nutritional requirements of the termite. There was a popular theory going around, mostly by a man named Cleveland, that termites could affix atmospheric nitrogen and thereby create their own source of protein. Well, we found out they couldn't. They have a big gut filled with microorganisms, which digest cellulose and reduce protein in the end. If you treat the termite with oxygen under pressure, you can destroy all these organisms. I did that to termites and took careful studies on their body weight, and that's published among the early publications. It did me a lot of good because I got interested in nutrition, and I used to circulate around the people on the Berkeley campus. We had Dr. [Herbert McLean] Evans, who ran an institute [Institute of Experimental Biology] on the top floor of the Life Sciences Building, which was my headquarters.

Hughes: How did that work out?

Scott: Well, it worked out that termites needed almost everything but vitamin C, as I remember the results, to survive as a population. We later did some work on the water control or the water environment of the termite. *Gustacolis*, the local California termite, can't survive in a very dry place. But there's one that's in Nevada, *Termopsis nevadensis*, which can survive in very dry areas. We did a paper on that.

Hughes: When you say "we"...?

Scott: Cook and I.



Hughes: You were working in Evans's institute. Was he giving you space as well?

Scott: No. I would go cadge purified casein, and things like that, from the people in the institute.

Hughes: This is in the mid-Thirties? You had just received your bachelor's?

Scott: Very early in the Thirties. I hadn't received my bachelor's degree by then. Poor S. F. Cook came back and he'd forgotten what I was supposed to do in the summer for him, that I'd done already. I couldn't interest him in it very much, but we eventually published it.

Hughes: Was that your first publication?

Scott: One of the first, yes. I did some photodynamics studies with Harold Blum, who was in the department. We were able to make the roots of barley plants bend in light sources. I found that very interesting, but I was no match for Harold Blum, and his mathematics were just way beyond me. I did publish a paper with him.

Hughes: Well, I notice that from 1934 to 1937 you were "Research Assistant" at Berkeley. In what laboratory?

Scott: That was in the physiology department. I had the job of running the tissue room and preparing all the animals and other necessary laboratory things for the students [in] both the medical class and physiology.

Hughes: At that stage had you ever thought of going on to a higher degree?



Scott: No, not really. I was naturally thinking of an M.D. degree. I was never really able to give that ambition up completely.

Hughes: Were you working with the idea of accumulating some money to go to medical school?

Scott: It wasn't possible. Whatever money I got I went out and spent until it was all gone, and then suffered the rest of the month. I didn't have any way of keeping myself dressed properly. My socks used to end at about the shoe line. I was very poor.

Hughes: Were you doing any research while you were working at your job?

Scott: All the time, yes. I had a continuous series of projects.

Hughes: Were you still working with Cook?

Scott: Yes, he was there, available all the way though until I got my Ph.D., which I did mostly at Crocker Lab.

### **Early Research Using Radioisotopes and Radiation**

Hughes: When did the connection with radioisotopes and the Radiation Lab occur?

Scott: Well, we're just about coming to that. I went to a beer party with a bunch of physicists that lived in the upper portion of Durant Avenue. I got to talking with them about the cyclotron. I was interested in blood and nutrition, so I asked them what elements they could make that were biologically interesting, and it turned out our best bet was radioactive phosphorus.



Hughes: Do you remember what year this was?

Scott: It was after '31. The paper, which was one of the first that came out of the Radiation Laboratory, was published in '37. But publications took a long time.

Hughes: Now is that the phosphorus study in chickens that you are talking about?

Scott: Yes, we did the study on the effect of P-32 on the white count of the chick.

Hughes: Now, who is "we"? Is that Cook again?

Scott: Well, Cook was in there, in sort of a... Well, he's dead now so he can't defend himself, but my impression of it [was] that he was forever smoking cigarettes. He had a pile of *Saturday Evening Post's* about three feet high, and his memory, retentive powers, weren't the best. He'd sit there and read these damn magazines day after day, smoking cigarettes.

Hughes: You mean in the lab he would do that?

Scott: Yes, in his office. He had the little corner office on the right-hand entrance to the Life Sciences Building. He had a couple of labs there and we had some other general facilities.

Hughes: So he was reading while other people were getting the ideas and doing the work?

Scott: Right.



- Hughes: Who was involved with that phosphorus study in chickens?
- Scott: Well, the physicists were people like Edwin McMillan, who was very much in the act in those days, and Luis Alvarez, whom you probably know. He just got a prize.
- Hughes: They were in charge of producing the isotope?
- Scott: They were running the original 37-inch cyclotron, which was the biggest mess of gum and stickum and wires and cages that you could see, but it ran very well. They ran it all day and they made 14 microcuries—which is a very small amount of radioactive phosphorus—for me.
- Hughes: Why did they become interested in doing biological experiments?
- Scott: Well, they had this machine and they were interested in doing anything. I think they were probably interested in helping me, too. We went through Professor Lawrence for most of the support on that. He finally threw me out of the lab, because I was such a pest, wanting more phosphorus to feed those chicks on.
- Hughes: I've read that Ernest Lawrence, in the early and mid-Thirties, was very interested in applying the cyclotron to biological and medical purposes.
- Scott: He was, because that's the only source of money he could get. Things were very tight in those days.
- They later made me more radioactive phosphorus. He was sitting in his office, I guess, and I had a lot of proposals to make, for things we could do with radioactive phosphorus. He was at loose ends, apparently, and brought me back into the fold. He had an associate who's



died very recently, by the name of Donald Cooksey, who ran things. He was the first lieutenant, I guess, in the laboratory.

Hughes: Did you have to go through him to get isotopes?

Scott: They got so used to me that I used to get them by winning small battles with Edwin McMillan.

Hughes: You'd just go up and pound on the door and say "I need some more phosphorus"?

Scott: I didn't want the target to be made out of brass; it just louses up the radiochemical preparation which follows it. I wanted aluminum. McMillan said, "Aluminum isn't going to do any good. It dissolves very rapidly in acid solutions." And I said, "No, it doesn't." And he said, "I'll show you." He got some aluminum and stuck it in a concentrated nitric acid and nothing happened. His face was very red, and he's been needling me all through these years about one thing or another. I don't think he ever forgave me for giving a big chemist a lesson in radiochemistry.

Hughes: Well, it probably made a bit of a difference, too, that here you were just a fairly recent bachelor's degree holder and you were showing up somebody much further along.

Scott: Well, that's partly true. He was from the California Institute of Technology, and they have an excellent staff down there. McMillan is an excellent man, but....

Hughes: Was he largely in charge of the isotope production in those years? What about Martin Kamen—when does he come in?



Scott: Martin Kamen was very busy in that area. Most of that production came on later. We had one technician in the old Radiation Lab, where the 37-inch cyclotron was, which was an old wooden building next to Gilman Hall, the chemistry building. The only person around that did any work in which I was involved was a Miss Condit—I've forgotten her first name or where she is. Maybe she's not around any more.

Hughes: What work did you do with her?

Scott: Well, the other big thing that happened to me was that Ernest's brother, John Lawrence, got very interested in radioactive phosphorus, and he knew quite a lot about transmissible animal tumors. We had a leukemia, or a lymphoma, which you could transmit to certain strains of mice. He was at Yale at the time, and he brought a lot out and got me started with leukemic mice and mice with solid tumors, lymphomas.

Hughes: What were you trying to find out?

Scott: I was trying to cure cancer.

Hughes: So you were looking for something that would localize.

Scott: Yes.

Hughes: What happened from there?

Scott: Well, we had terrible equipment problems at that time and measurement was always a problem.

Hughes: Excuse me, but were you now actually working in the Rad Lab or did you still have a niche in the physiology



department?

Scott: Well, both.

My first memory of Joseph Hamilton was when we were weighing out some mouse tissues to assay for radioactive phosphorus. Joe came in and sat down on a stool in the lab behind us and introduced himself. He was from the neurology department at the UC Medical Center. Later Joe denied any of this, but I still remember it.

Hughes: Was that very early?

Scott: That was very early, that was a couple of years before the first publication on the treatment of mice for leukemia with P-32. By carefully judging the dose, I was able to cure about 30 percent of my animals. They turned out sterile, and it was a very careful dosage routine I had to follow.

Hughes: How were you determining the dosage?

Scott: Well, I got a standard from McMillan. McMillan said, "This standard emits 500 beta particles per minute," and I built myself a standard out of uranium oxide, which was a powder, and got it to discharge the electroscope at the same rate. Then I said this standard emits 500 particles a minute. Since the microcurie is the disintegration rate of a certain amount of any isotope, all of our radioactive phosphorus was based on that standard.

Hughes: How did you determine biological effectiveness?

Scott: By blood counts, mostly. If they fell out of sight, why, usually the animals would die.



Hughes: Did you find there was significant localization?

Scott: Well, in the bone marrow. Those days were the very beginnings of radioautography. Other people in the lab, whose names I've been trying to remember, who were physicists, were helpful too.

Hughes: I've read something stating that Hamilton was responsible for developing the technique of radioautography.

Scott: No. We had Dorothy Axelrod Heller, who made many radioautographs, mostly with plutonium because it's an alpha emitter and you get beautiful pictures.

Hughes: But that was later, wasn't it? That was the war.

Scott: Much, much later, yes.

Hughes: Do you know anything about who was responsible for the initial development of the technique? Was that done at Berkeley?

Scott: It was done at Berkeley by a young Belgian [Charles Pecher].

Hughes: He was killed in the war?

Scott: No.

Hughes: That must have been sort of a cover-up that I read, then.

Scott: I guess it was. I don't know what was wrong with him. He had a very nice wife. He and his wife lived in a little one-room apartment deal that I used to live in. The landlady



would come busting in any time of day or night and close the curtains to protect the place, or something like that. It was one of those fishbowl jobs, you know. You couldn't relax with her in the area.

Hughes: Well, what was the response to your work with the P-32? Was there excitement about a potential cancer cure?

Scott: Well, we were very enthusiastic and other people were, too. The *Chronicle's* emeritus science writer, Gobind [Behari] Lal, wrote a one-page dissertation on radioactive phosphorus. I have a copy of that among all these papers around here—a reprint out of the paper. I had people come in through my office when I was at San Francisco. And every once in a while I met the original person who did the original work. So I had this thing on the wall and I just said, "Well, you're wrong. I did the original work and go read it for yourself." I think he became Sharp Electronics—who thought he was the first investigator, but that wasn't so.

Hughes: Who was working with you on these early P-32 studies? What role was John Lawrence playing?

Scott: John Lawrence was really the director of the project, and his brother Ernest, who was very fond of his little brother John.

Hughes: What sort of accommodations did John have at that stage? You're still talking about the period before he comes permanently to Berkeley? He's still going back and forth to Yale?

Scott: Yale, yes. Well, he came out one year on a full-time basis, and he couldn't live anywhere. We tried him at the



Faculty Club and he was bitten to death by fleas.

Hughes: He lived with his brother for a while, too, didn't he?

Scott: I don't know too much about his comings and goings. Somewhere along that line he married Amy Bowles, who was a very lovely girl, and very wealthy, incidentally.

Hughes: That was in 1942, so that was a little bit later. Were you working with Paul Aebersold?

Scott: Well, Paul was very helpful in the lab, and he was a very [efficient] guy. Robert Stone didn't give him the credit he deserves. He ran that million-volt x-ray tube from the beginning for Robert Stone, which he mentioned in his letter. He later became the ambassador of isotopes for the Atomic Energy Commission. I had a very pleasant association with him.

I got interested in neutrons—this was in the late Thirties—and I compared the effects of neutrons on mouse tumors and how [they] interfered with the radioactive phosphorus deposition in their tissues. Neutrons were very useful in causing tissue damage. That was published. Paul did the neutron dosages for me, because I wasn't in any position to do it.

Hughes: Had they begun cancer therapy at that stage?

Scott: Yes, this was during the period where we had the 60-inch cyclotron at Crocker Lab.

Hughes: Can you tell me about the decision to try neutron therapy on patients? Were you involved with that in any way?

Scott: Well, when we found out those funny particles were



neutrons—no one knew what a neutron was in those days—everybody got in the act. I did my study, which I'm still very proud of. It's a very useful tool. Many years later some Russians thought they had discovered this effect of neutrons on the distribution of radionuclides in the body, and they published their great experiment as proof. I sent a reprint to some place way off in Russia somewhere, stating what we had done, and it was much earlier than theirs by, oh, a generation practically.

Hughes: What do neutrons do to the distribution of—?

Scott: Well, they're about six times more effective than x-rays, as a rule of thumb that we use, ion for ion or dose for dose.

I heard from those Russians. They were very pleasant about the whole thing.

Hughes: Well, getting back to the neutron therapy, were you a part of that operation?

Scott: No, I was just in the same lab. Let's start out with Crocker Lab. I don't know why they tore it down. They claimed it was too heavily contaminated with things like plutonium and so on.

Hughes: But you don't think that was true?

Scott: Well, I don't think so, no. I was very careful in handling stuff like that. I insisted on recovering everything I gave in the way of doses of radioactive elements to animals. And you can knock on wood, but I'm one of the pioneers that doesn't have leukemia.



### **Crocker Laboratory**

Hughes: I'd like to talk about that a little bit later. Let's talk about Crocker.

Scott: Well, the people at Crocker were people that John Lawrence attracted. Most of these people went through my hands, because I could show them what we did with our equipment. There was Lawrence Tuttle. He worked with me for quite a while.

Hughes: Where did he come from?

Scott: He was from UC, in the Department of Agricultural Engineering or something like that.

Hughes: Had he previously been interested in radioisotopes?

Scott: No, he just came in looking for part-time work, and they put him on as a radiochemist-biologist type.

Hughes: Were you by this time a full-time employee of the Crocker Radiation Lab?

Scott: For \$50 a month.

Hughes: Was that brought about by your association with John Lawrence?

Scott: Yes.

Hughes: It was logical for you to come along with him?

Scott: Yes, I was doing his work. We were then treating patients



with leukemia with radioactive phosphorus. My job was to do the excretion bit with the stools and so on.

Hughes: How was that decision made, to treat the first patient? I believe it was in 1937.

Scott: Yes, it was on Christmas Eve. I remember I got a call at home. I was supposed to come in right away. I don't think the Lawrences ever really trusted my radiochemistry, so they got Tuttle to do radiochemistry for a while.

Hughes: Why would they not trust your radiochemistry?

Scott: I don't know. I wasn't a chemist, for one thing.

Hughes: But Tuttle wasn't either.

Scott: No, he wasn't. I'm trying to think of the other people we had around there. There was Martin Kamen.

Hughes: He was doing a lot of it, wasn't he?

Scott: Yes, he really knew chemistry.

Hughes: Well, that was his background.

Scott: Yes.

Hughes: Was Seaborg in the picture at all at that stage?

Scott: Well, at a distance. He was early involved in the Atomic Energy Commission. But Crocker Lab in the early days was a very strange place. We had all these characters throwing their weight around. We had an Alfred Marshak,



who was a very fine cellular physiologist. I read in the paper where his sister was looking for him. She went to Berkeley, and I guess he wasn't anywhere near there. He'd not been there for years. I used to run into him every once in a while. He always hated John Lawrence, because he [thought] he gave him a raw deal.

Hughes: On what specifically? Do you know the background?

Scott: Well, Marshak had a fellowship, which was quite a nice fellowship, and John just let him run down and had nothing else to offer him.

Hughes: Was this in the war years?

Scott: Before.

Hughes: Wasn't there some political controversy, too?

Scott: Well, the only political controversy I heard about was the Lawrences' trying to move the first year of medical school to the [UC] medical center [in San Francisco. On December 4, 1980, Dr. Scott said that it would be more accurate to say that the Lawrence brothers wanted to establish the medical school complex in Berkeley.]

Hughes: But that was later, was it not?

Scott: Well, it happened later but it was continuing on for a whole generation.

Hughes: How early do you remember them trying to do that?

Scott: Well, in the late Thirties, I would say, is the first I became



aware of it.

Hughes: Why were they so interested in having it moved?

Scott: Well, everyone was very critical of the UC medical center, with its old-fashioned school and its old-fashioned faculty.

Hughes: Well, let's finish with the P-32 therapy, because that's an interesting episode. Can you tell me about the first patient treatment on Christmas Eve? Did you go in when you were called at home?

Scott: Well, I went in and did my job. I wasn't interested in that; it was kind of a big grandstand act.

Hughes: For whose benefit?

Scott: The Lawrences, John and Ernest.

Hughes: Was this again to attract funding?

Scott: Yes, and fame.

Hughes: What was the basis for making the step from the animal experiments to human therapy?

Scott: Our results on mice in the early days, on monkeys—we eventually had some monkeys.

Hughes: I believe that P-32 therapy for leukemia petered out in the war years. Was that simply because it was realized that it was not effective?



Scott: Well, I don't think that it ever got a fair chance. If you can cure mouse leukemia with P-32, you can cure human leukemia. But no one had guts enough to give them the amount that they needed at the right time.

Hughes: You mean they were underdosing?

Scott: Underdosing and letting the disease keep ahead of them. I established the lethal dose of P-32 in rhesus monkeys. And Mrs. [Anne] Low-Beer did the cell counts for me. A dose, if I remember correctly, of about 2.5 millicuries per kilogram of body weight would just about kill half of them.

Hughes: Why were the Lawrences afraid to take that one extra step to give a really effective dose?

Scott: Well, I don't know. Part of it was the shortage of P-32; we had trouble making enough on the newer cyclotron that they built and put in Crocker Lab.

Hughes: The 60-inch.

Scott: Yes.

Hughes: And yet the 60-inch was a better producer of isotopes than the 37-inch, wasn't it?

Scott: Oh, much, yes. Much better.

Hughes: But it still wasn't enough for adequate...

Scott: Well, it just didn't have the output to do it.

Hughes: What was the response of the medical community, and



specifically the medical school in San Francisco?

Scott: They were interested in what we were doing. A man's name who pops up in Dr. [Robert S.] Stone's tape, Stacey Metier, was in there. And I remember S. F. Cook going to San Francisco with me, with our leukemia studies and the effect of P-32 on leukemia and so forth. I remember talking to Metier, who was just a young man then, and one of the therapists. He got leukemia cases and so forth.

Hughes: Is that where you were actually treating the patients?

Scott: No, they were ambulatory, most of them, and they came in to John Lawrence for their workups. Lawrence Tuttle, who was kind of a character anyway—I'm very fond of him; I've lost touch with him—he used to talk to these patients and kind of play doctor. John thought it was me, and he blamed me for it and got very upset.

Hughes: John thought that that was *his* role as a doctor?

Scott: Oh yes, a doctor-patient relationship. We went right by the book on that.

Hughes: Was there a problem in those days about patient treatment? I know that patient treatment on the Berkeley campus became a big issue from the mid-Forties onward.

Scott: Well, I don't think any patients got treated on the Berkeley campus except the one that went into John's laboratory there. It became Donner Lab. Then they had a pavilion at Cowell Hospital.

Hughes: But in these early days all the patient treatment was done at the medical school with medical faculty presiding?



Scott: Down in the physics department at UC, in the Radiation Lab, under John Lawrence's direction.

Hughes: That was all right with the medical school? They weren't worried about the fact that physicists [were involved with patient therapy]?

Scott: Well, John Lawrence was never accepted in the medical school. He wasn't a board member of anything, which they always looked down upon there. You've got to be certified in something, hopefully radiology. But the radiology department just wouldn't have him.

Hughes: Why was that?

Scott: Dr. Stone didn't like him.

Hughes: Was that just a personal thing?

Scott: I think it was kind of a personal thing and a political thing.

Hughes: Did Stone look upon Lawrence as a threat to his monopoly over all forms of radiation?

Scott: No. I don't know if he felt that way, but Stone really owed Ernest Lawrence a lot. He made the 60-inch cyclotron available for the neutron therapy that Dr. Stone did. There was another man, who worked with Stone, by the name of [John] Larkin, who was upset by all of this, because Larkin thought it was his neutron therapy. Dr. Stone's wealthy patients used to roll up with with chauffeurs and their iced champagne. In the laboratory they had a big field day that afternoon; we all got smacked on champagne and I thought it was wonderful.



Hughes: What was the breakdown of labor? You have John Larkin, who was an M.D., and Robert Stone, and then John Lawrence was involved in the early days in that neutron therapy. What were the three M.D.'s doing?

Scott: Vying for power. It's as straight as that.

Hughes: Can you expand on that?

Scott: Building on their own personal ambitions and regaining and maintaining control over the program.

Hughes: How was this vying for power expressed?

Scott: Well, usually by the type of papers that people would give at scientific meetings.

Hughes: What do you mean when you say that?

Scott: Well, I was thinking of Dr. Edith Quimby, who finally wrote a textbook on nuclear medicine. She got up at a meeting we had in the early days and said that radioactive phosphorus wasn't anything that you could use, that it was worse than an x-ray machine, and you could do anything with x-ray machines that you could do with radioactive phosphorus or anything else. John Lawrence was sitting beside me, and this was a slap to him [of] the worst dimension. Edith Quimby eventually became grandmother of nuclear energy, and she'd go around and give courses to radiologists who were never really well prepared in the field anyhow.

Hughes: What was John Lawrence's response?

Scott: He was very upset but he didn't do anything on the



surface about it. There was a lot of under-the-table negotiating going on.

Hughes: About what?

Scott: Usually about the control of things.

Hughes: Now, Crocker Lab opened in 1937. Can you tell me how people came to be employees of the lab—how personnel were attracted?

Scott: Well, mostly by word of mouth. The people we had there were people supported by a grant, like Dr. Marshak. I've forgotten what fellowship he had; I think it was a Markel fellowship.

Hughes: Had he been on campus before Crocker opened?

Scott: No, he was brought in as an out-of-state person.

Hughes: Now, who would have been responsible for that? Would that have been one of the Lawrences?

Scott: One of the Lawrences probably accepted him as a fellow. I helped him quite a bit in the area; I helped him get a car. Anytime anything happened, I was the first to learn about it. He left in an era [in] which he was very dissatisfied with the performance of the Crocker Lab and what it could do for him.

Hughes: Why do you suppose that John Lawrence let his grant run down?

Scott: I don't know. I wasn't in on any of the decisions which



were made at the time. I eventually left the lab completely.

Hughes: That was after your Ph.D.?

Scott: No. Before.

### **Private Business**

Scott: As I said, I was always interested in nutrition. And I needed an income a bit greater than \$50 a month. Even in those days that wasn't very much. So I built a home in Richmond. I built the place lock, stock, and barrel. I bought some cheap lots out in Richmond. On the Fourth of July we started digging the foundations. It was hot, and oh, that soil was very hard. I built a little house with one bedroom and a great big living room, plumbing and fireplace and everything. And we were in before Christmas. That was my half-time job, transferring my activities over to the sale of laboratory animals.

Hughes: I notice on your curriculum vitae: "1939-1942, private business."

Scott: That was it. I found out how to raise laboratory mice in large numbers by using a diet that I—I can't say invented, but developed. I found out that mice needed about five times the vitamin intake that dogs did. Everyone raised them on dog pellets, which was a very poor, unrewarding thing to do. I got it developed to the point where I could get one mouse for every breeding female I had in the laboratory, which I had in a little building back of the home I built.

Hughes: To whom were you selling the mice?



Scott: Mostly to the Navy. They went to Dr. [Albert Paul] Krueger's lab primarily. He developed a lot of influenza vaccines and I don't know what else with these mice. It got so big that I was selling two and three thousand mice per month.

Hughes: Is that the Krueger who now is writing a lot about air ions?

Scott: Yes, I think it must be. He's quite old now—he's 20 years older than I am anyway. He's been a bacteriologist at UC for many years.

Hughes: Then I'm sure it's the same one. So the business was doing well?

Scott: Well, I made a lot of money.

Hughes: Why did you leave it?

Scott: Well, I had a nervous exhaustion experience. I just wasn't happy doing anything with anybody or anything else. So I sold it to a poor fellow who I don't think understood how to do a thing like that.

Hughes: And then what did you do?

### **Return to Crocker Laboratory**

Scott: That was about the time we opened the doors at the Crocker Lab again. I was hired by Joseph Hamilton, who headed up the secret W48-A engineering of the Manhattan District.

Hughes: Well, the Crocker had been opened for a little while before



the war. I think it opened in 1937. The Donner opened in 1942.

Scott: I never had anything to do with Donner Laboratory or their people.

Hughes: How separate were the two? From talking to people it seems to me that they were run as strictly independent entities.

Scott: They were. They were both under John Lawrence's control, I think. Cornelius Tobias was his man. Hardin Jones became his man. Hardin Jones started out at the Crocker Laboratory.

Hughes: Did you have any contact, both of you being physiologists?

Scott: I had Hardin as a student, more or less. I could never get him to do anything right; his tracer studies weren't, I thought, adequate. He just wanted to do his own thing.

Hughes: Did you have any dealing with him later on in life, when he was established in Donner Lab?

Scott: No, none at all, really.

Hughes: Going back to the opening days of Crocker Lab. John Lawrence, of course, was there until Donner Lab was opened. And Hamilton was there from the very beginning?

Scott: When John moved out of his office in Crocker, Joe Hamilton moved in with this new secret project.



Hughes: How did that come about? Do you know why Crocker and Hamilton were chosen to run the fission metabolism experiments?

Scott: Yes. Well, Joseph Hamilton had the support of Ernest Lawrence. He became quite capable in the physics necessary to run a cyclotron. So they turned the 60-inch cyclotron over to him.

Hughes: Do you know how he became interested in radioisotopes? I know he was involved relatively early, before he even came over on a permanent basis to Berkeley.

Scott: Well, I told you of my memory of his first visit to the lab. He was just a young resident in neurology interested in doing something interesting.

Hughes: And you think that visit was what precipitated [his interest in radioisotopes]?

Scott: Well, he was hired on the staff by then. They gave him a laboratory that he could run. He was interested in the metabolism of various elements. I was a guinea pig of his. He worked out the excretion of bromide in the body, and I saved a urine sample every time I had one, until it seemed wrong to throw them away. You know that funny feeling.

Hughes: Well, even before that he had done some radiosodium work with Stone. In fact he published in 1936, I believe, before he was really over on the Berkeley side of the Bay.

Scott: Well, Hamilton always was vacillating back and forth.

Hughes: Do you know anything about those early radiosodium experiments?



Scott: I don't know anything about them. I don't know what he did with Stone, except what Stone says. The main interest had to be radioactive iodine in those early days. It's a fission product, as you know. Iodine-131 with an eight-day half-life was really developed from the uranium materials that were in the nuclear reactor, in the pile. The Atomic Energy Commission began shipping out batches of I-131 to any of the subcontractors, which included us.

When I got it at the Radiation Lab, at Crocker Laboratory, I was quite a bit in control of laboratory policy. I interested Earl Miller in using radioactive iodine to investigate thyroid disease and so forth. It has become quite a famous laboratory. They named it after him when he retired. Kind of a gift you get out of the university; takes 40 years.

Hughes: I know Hamilton continued his iodine studies after the war. But a lot of his work was on the naturally-occurring elements—things like the bromide-chlorine studies that he'd done before the war, sodium. But that pretty much dropped out of the picture after the war.

Scott: Well, I guess it did. We were all mainly concerned with atomic bombs.

Hughes: Why was that? Just because the AEC thought it was important and you had the techniques?

Scott: Well, that's where our money came from.

Hughes: Were you in on that very early war research? Do you know why Hamilton and the Crocker Lab were chosen? Was it simply because of the 60-inch cyclotron?



Scott: Yes, primarily, because when I got in the group it was really secret; I wasn't supposed to know anything. You can't help but find out, just by osmosis. When he hired me, I was there as a Laboratory Technician I, I guess.

Hughes: Had he sought you out? How had you met up again after your episode with animal breeding?

Scott: I don't remember. I left there under kind of a cloud. I didn't get along very well with John Lawrence. Ernest, of course, his big brother, was very nice to me.

I made such a mess in the laboratory without a hood for a muffle furnace. I made these disgusting smells, muffling human feces in open air. There was a narrow little walkway between Crocker Lab and the old Laboratory. I can see Don Cooksey in the back of my mind, coming in one day. He was grabbing his throat, coming in the door, wondering what the terrible smell was. I said, "You can't do this kind of work without a proper hood. And I have to do it, so this is what we're faced with." Well, he went over to Ernest Lawrence right away.

Hughes: And you got a proper hood.

Scott: And I got a proper hood. Looked like a steamboat.

Hughes: Were your differences with John Lawrence mainly [based on] personality?

Scott: I thought it was based on a matter of performance. I expected performance out of him, which he was incapable of giving.

Hughes: You mean scientific performance?



Scott: Well, yes, that, and in everything. He was a terrible laboratory man. And he was also very late on many occasions. I'd show up walking the floors trying to be present when he needed something and...

Hughes: When you say "terrible laboratory man," do you mean that he just wasn't very skilled in laboratory procedures?

Scott: He was very undexterous. One of the biggest breakthroughs of localizing P-32 in an animal tumor, which we were using one of Ernest Lawrence's fancy instruments to measure, turned out to be some contamination on John's thumb and [the instrument] went right off the scale. That kind of thing.

Hughes: What effect did his lack of performance have on the way research was conducted at Donner Lab?

Scott: I think it held him up, especially when he went to the UC Medical Center or anywhere else. People would tend to downgrade him because of lack of experience....

Hughes: Do you think there was any tendency, as time went on, for him to go less and less often into the lab and become more and more concerned with administrative duties?

Scott: Well, I saw so little of him in those years that I don't know what he did with his time. He also was pretty heavily involved with Cornelius Tobias and Hardin Jones. They had a high-altitude laboratory [University of California White Mountain Research Station] there of some sort. That was all part of the medical physics group. S. F. Cook again took charge of that, just by expressing his personality.



Hughes: How did Cook express his personality?

Scott: Well, he controlled the whole thing for many years, both budgetarily and research-wise.

Hughes: He had the money?

Scott: Well, he had control of the grant. The medical physics group, which was mostly physicists from the Berkeley campus and some other people like Cook, controlled medical physics in the University of California for quite a few years.

Hughes: Is this in the postwar years that you're talking about?

Scott: Well, this is mostly during the war.

Hughes: So they had the grants and consequently could call the shots, is that what you're saying?

Scott: Yes.

Hughes: How did John Lawrence react to that?

Scott: Well, I really didn't know him well enough or have enough to do with him to see what his reactions were. We kind of lost him during World War II.

Hughes: And never got back to him after the war?

Scott: No, I've had some very nice chats with him. You meet him in a hallway near an elevator or something, but...

Hughes: What about Hamilton and Lawrence—what sort of a



relationship did they have?

Scott: With Ernest Lawrence it was excellent. He and John didn't get along too well.

Hughes: What was the basis of that?

Scott: Struggling for a place in the sun, is about all I can say about it.

Hughes: Even after each had his own laboratory?

Scott: Well, that was more or less true; John was usually involved, one way or another. I can't help you much there. My memory is very clear on what happened when I went back to Crocker Lab after my animal business success.

Hughes: Well, that would have been about the time that Donner...

Scott: Was opening up.

Hughes: So that's when you lost track of John Lawrence.

Scott: Really it is, yes.

Hughes: Is it your impression that Hamilton was left pretty much in charge of the ship, so to speak? He was making the decisions?

Scott: Yes, he really was. He had the support of the people back in Washington who were responsible for the Manhattan [Engineer] District. And he had a great deal of influence with those people.



- Hughes: How do you reach the stage of having influence with people like that?
- Scott: Mostly by being there and talking to them when they had a need.
- Hughes: At least in the early days, was it true that the 60-inch was about the only machine around which could produce the isotopes for the experiments that the AEC and the Manhattan Project needed?
- Scott: Well, they had a facility at the University of Chicago. Now they were nuclear-reactor-minded. But I think they came mostly to Joe Hamilton for help. Now, two people figure in this. One is Robert Stone, who went to what we call the Metallurgical Laboratory Project, as director of medical research, I would guess. And the other person that shows up around here is Stafford Warren, who was one of my bosses.
- Hughes: What are their roles?
- Scott: Well, Stafford Warren was a full colonel in the Manhattan District, and he spent his time traveling around the country, breathing down the backs of people like me, who was given the first plutonium to experiment with, to find out where it went in the body.
- Hughes: How did that happen?
- Scott: Well, it was primarily the relationship between Hamilton and Seaborg, who was right up at the top in producing new elements and isotopes.
- Hughes: Plutonium had been produced for the first time on the



60-inch, had it not?

Scott: Well, I guess it had, for physical experiments. But that wasn't enough to use in animals. I was given, through them, 11 milligrams of plutonium. It's in that plutonium reprint.

Hughes: Now, who would have been responsible for making a decision to have plutonium set aside for biological research?

Scott: Hamilton, through Seaborg.

Hughes: Would Stone have played any role in that?

Scott: No, he was completely out of that part of it. He was in Chicago and later Oak Ridge.

Hughes: Do you suppose Hamilton would have written to Seaborg and said, "Please give me a piece for biological experiments"?

Scott: Yes. That's how we got all the fissionable materials which we eventually did [research on] and published on. That included americium and neptunium and so on.

Hughes: Was that done by telephone?

Scott: You couldn't make a phone call and have it stick. It was strictly top-secret stuff. [Interruption]

Scott: Well, I went back to the Laboratory in October 1942, and there were these two chemists that I'm very fond of, [Roy] Overstreet and [Louis] Jacobson. They were very good soil chemists, and they were used to working with trace



minerals and elements. But they wouldn't work on anything unless it was a white ash, which was one of their conditions for me. [If] I brought something in dripping of blood, they would say, "Don't bring it here. Take it away and make it into a white ash."

My friend Overstreet died not too long ago, of cancer. He was a chain-smoker, and he had cancer of the lung.

Hughes: How did they come to be at Crocker Lab?

Scott: Joe Hamilton hired them, found them as part of our group.

Hughes: What was your job during the war?

Scott: I was the major flunky and tracer man. I was surprised to find out that no one knew how to do a decent tracer study when I came back to the lab.

Hughes: Including Hamilton?

Scott: Including Hamilton. He knew how to do it, but he didn't have the patience for that, measurements and so forth. So I set up all the tracer studies and directed them. I finally wound up with a staff of about 14 girls, and we had our ins and outs as all personalities will. This is where Patricia Durbin came along. I would never tell this to anyone who didn't know it, but we did hire her as a dishwasher. She did come along and develop beautifully as a scientist.

Hughes: What was Hamilton's role?

Scott: He was her boss.



Hughes: But in the lab as a whole? If you were mainly doing the tracer experiments, what was left for Hamilton?

Scott: Well, about 5,000 plutonium analyses. I was pretty slaphappy. He said, "Well, what are you doing all that for, Scott? You can go hire some people to help you." And that was new to me. I hired quite a few people. One of them was Durbin. One of them was Josephine Crawley, who was a lovely girl. I only know of what has happened to her since Patricia Durbin told me. She had a complete mental collapse over the project [injection of plutonium into a welfare patient] in general and her part in it. She was one of my right-hand people.

Hughes: You mean several years afterward, she had the collapse?

Scott: Yes.

Hughes: Why? Over the project?

Scott: It came on much later. She was a devout Catholic girl. I was emotionally interested in her, involved. I was very fond of her, but there wasn't anything I could do about it; I was married already. We had a close relationship, but it wasn't any more than that.

Hughes: Well, what was Hamilton doing?

Scott: Buzzing around the country mostly. Picking up unusual radionuclides that he could bring back to the lab and toss into the hopper.

Hughes: What about the cyclotron itself? Who was responsible for keeping that going?



Scott: He was.

Hughes: Well, what happened when he was off buzzing around the country?

Scott: Well, he left it with the crew. We had a regular cyclotron crew. We had some young physicists. One of them was the son of the chairman of the physics department, Dr. [Raymond] Birge, who was kind of a curmudgeon character. He'd issue you a key to LeConte Hall and charge you a dollar. Then if you lost it, he'd give you hell, and if you found it again, he said, "Oh, you only found it because you could get your dollar back." That kind of a guy.

Hughes: Was the crew solely responsible for scheduling and operating?

Scott: Yes, if I needed something I'd go to the chemists, and go to the crew. I don't remember the members of the crew too well, as to what their names were.

Hughes: I know there was other work going on, aside from the fission product metabolism studies. Hamilton had a contract which he shared with [Dorothy] Axelrod. Do you know about that?

Scott: Yes, that had to do with radioautography of fission or fissionable products, or other things.

Hughes: You had nothing in specific to do with that project?

Scott: I worked with her directly. I usually gave her all of the specimens. If she needed a lung or a liver from a rat, I gave it to her.



Hughes: Was there anything else going on in the way of research in the war years?

Scott: Well, we were using the cyclotron to make fission products. I think the first thing the chemists produced that we used was radioactive yttrium, which is a fission product. We were bombarding the uranium, and Joe Hamilton was making the world supply of polonium. He exposed himself to polonium, which is one of the things that probably led to his leukemia that killed him.

Hughes: You mean accidentally exposed?

Scott: Yes, in one of the laboratories in the chemistry department. The whole thing got away from him and blew up or burned up. Something like that.

Hughes: So you think that the exposure to radioisotopes was a factor in his leukemia?

Scott: Well, yes, and the fact that you couldn't keep him out of the control room or the bombardment area where the beam came out. If somebody wanted something, he just couldn't rush in there fast enough. There's a lot of residual radiation from a thing like that. I could pick it up on my instruments that I ran in LeConte Hall, which was in the basement, and it had to shine down from the cyclotron all the way down into that building.

Hughes: Why did he take such risks?

Scott: Impatience, I think mostly. He was a very impatient man. I think he realized that he had already had it in the way of radiation exposure, because of accidental things and so forth.



Hughes: So he didn't think precautions were necessary.

Scott: Not enough, and I jumped on him for it. We all got after him. It didn't help.

Hughes: How was he in regard to the safety of other people?

Scott: Well, he was kind of thoughtless of it, I think. He never worried about whether I had any plutonium in me or not. And it turned out to be one of the most deadly things you can get.

Hughes: I hope *you* were worried.

Scott: Well, I was being very careful in those days. I knew what I wanted to do, and how to do it right. And if you did it right there wasn't any radiation exposure.

Hughes: If Hamilton wasn't particularly concerned, then who was seeing to it that people in lesser positions in Crocker were taking proper safety precautions?

Scott: Well, I was very concerned. That was one of the reasons why I left Crocker in 1951, and moved all of my activities to the Radioactivity Research Center on the San Francisco campus. I went away and I never came back really.

Hughes: Because you didn't think that the safety standards were high enough?

Scott: Well, he wanted to do some very, what I thought were incautious experiments. I thought they were morally wrong.



Hughes: Are you talking now about the human experiments?

Scott: Yes.

Hughes: And he went ahead and did those?

Scott: Yes, he did, and he did the first one with my help.

Hughes: That was the plutonium?

Scott: Plutonium-248, which we gave to this nice man who was scheduled for stomach surgery. They were sure—Earl Miller, for example, was sure—that he had cancer of the stomach and his probable survival wasn't very great. He was 55, maybe, when I first found him. We injected him with plutonium-238, and the story of it is that he didn't have a cancer that anyone could demonstrate. Earl Miller got very upset with that and looked for days at slides of this man's post-op remains, and he just didn't have it. I got very interested in him as a person, and I contracted through the laboratory to buy all of his urine and feces, for which he would get a monthly check. We would go up once a week and pick it up in acid carbolase in various bottles we left up there with him.

Hughes: Did he know what was going on?

Scott: Never told him.

Hughes: What was the outcome?

Scott: Finally the laboratory wouldn't pay for his feces any more. He was in excellent health. His sister was a nurse and she was very suspicious of me. But to my knowledge he never found out, and he slipped through our fingers at



the age of 88. He died from something.

Hughes: Nothing to do with the plutonium?

Scott: He got many times the so-called lethal textbook dose of plutonium. Patricia Durbin knows more about that. She's kept up with his data.

Hughes: In those days it was possible to do experiments on human beings with such ease?

Scott: Yes, yes.

Hughes: What did it involve?

Scott: It involved getting a needy patient who had a known disease, or thought it was known. He came out of the clinic for us at UC. I took the plutonium over there and gave it to Earl Miller, who injected it into this guy.

Hughes: Later, other experiments of that nature were done?

Scott: There was one more that Pat reminded me of—an Australian child who received some fissionable product. I don't know; I think he got plutonium too. [He] was dying of leukemia when they got him. And I think he subsequently died in a very short time.

### **Human Use Committees**

Hughes: When did the human use committees put a stop to all that?

Scott: Well, they never really did. I was on the committee when it



was organized in 1948. We used to screen these applications, one by one.

Hughes: Now this was the committee of the medical school?

Scott: Yes. And it was through that committee that the University of California got permission from the Atomic Energy Commission for blanket authority on various and sundry radionuclides.

Hughes: So it was just a matter of this board meeting and making a decision in regard to each case?

Scott: We'd circulate each application around, and I guess I had quite a bit of weight in the committee. We went through a whole series of chairmen. The president's office in Berkeley used to have his secretary call me up and ask me, "Who do you suggest next?" So I went through a whole bunch of people, including Dr. [Glenn] Sheline, who is now a radiotherapist in the Department of Radiology. I think he's been loaned to the National Cancer Institute or something.

Hughes: Why do you think you had such authority?

Scott: Well, I was the only one over there who could really figure out in radiation doses what the experiment was going to give to the recipient and whether it was worth doing or not, or whether it was just a pie-in-the-sky thing.

Hughes: So this committee was only considering cases that were going to involve the use of radioisotopes.

Scott: Yes. On what we'd call an experimental basis.



Hughes: Would this include experiments that were projected from Donner Lab and Crocker Lab? Did you have control over the Berkeley campus?

Scott: No, what they did at Berkeley was finally none of my business, and I didn't want it. I didn't want to have anything to do with it, really.

Hughes: Do you know anything about how those decisions were made? Was there another committee somewhere?

Scott: Well, there was a state committee [Governor's Radiological Safety Advisory Committee] that was for the Northern California branch[es] of the University of California, and I was chairman of that for quite a few years.

Hughes: Would that have made case-by-case decisions as well?

Scott: Yes. I was responsible for letting one of my friends, Perry Stout up on the Davis campus, bury 215 millicuries of radioactive zinc around an apricot tree or something. The university found out about that later and they had convulsions, practically. I didn't think the experiment was all that much. It was out there by itself in a field. And I had a very [high] opinion of Perry Stout. He was an excellent man. He knew more about colloid chemistry than anyone I'd ever met. Unfortunately he died just a while ago, poor guy.

Hughes: The university was concerned because of the radiation hazard?

Scott: Their risk in getting sued by somebody for exposure or something like that.



Hughes: You don't know anything then about the problems that the medical physicists on the Berkeley campus may have had in regard to human experimentation?

Scott: I think you could get more information from Hardin Jones or Cornelius Tobias.

Hughes: Hardin Jones is dead. He died almost two years ago.

Scott: I didn't know that.

Hughes: Yes. He died suddenly. I believe it was a heat attack. He'd been to Australia on a rather rigorous trip and, from what I understand, died rather suddenly after that.

Scott: He used to sound off about marijuana. He got very moral towards the end.

Hughes: Well, do you think we've covered the war years sufficiently?

### **Bikini Bomb Tests**

Scott: Well, we're right up to Bikini.

Hughes: Perhaps that's the place to start. I know you had a [big] hand in that. Can you tell me how that arose?

Scott: Yes. Joe Hamilton and Stafford Warren recruited the staff for that. I was given a simulated title of major. I think I was a major. It didn't matter much anyway. They gave us a bunch of free clothing, which we sweated our hearts [out in], anything that's [as far] south as Bikini. I set up what they called a radiological laboratory on Hospital



Ship *Haven*. And this I was entirely responsible for running. We went through the surface Able Test—that was the bomb dropped from the air (I have some pictures of that, by the way)—and the Baker blast, which was an underground explosion thing.

Hughes: Does that mean that you were there two times?

Scott: I was there during both tests. Ran over a period of one month or two months, but I enjoyed it thoroughly.

Hughes: You stayed the whole time there for the two?

Scott: I stayed over, yes.

Hughes: What were you supposed to do?

Scott: I was supposed to give them an assay on anything they brought in they thought was radioactive. I was out in the beginning with monitoring instruments. Later on I had enough equipment in the lab to make our own detection instruments, like Geiger counters. Everything worked beautifully as far as I was concerned. I worked my tail off assaying samples.

I liked the Baker burst because it was the biggest tracer study that I was ever involved in. The Bikini Lagoon's a fairly good-sized place. We had samples brought in to us, which we ran, and plotted the distribution of the radioactivity in the lagoon. We did that by a radiochemical separation which was quite simple and always works. It was making an iron hydroxide precipitation of the sea water samples. We'd collect the precipitate and count that and there's practically no mass involved.

Hughes: Who is "we"?



Scott: Well, I had a few men from the Army assigned to me, and that was "we."

Hughes: And they presumably had chemical backgrounds?

Scott: No, they were straight out of the service; I had to train them. And some of them became very successful. One of them, Kermit Larson, a great big Swede from a northern plains state—I've forgotten now—became very useful to them at UCLA when they had a radiation lab there. Stafford Warren was always involved in various explosions and tests of one kind or another.

Hughes: Why Stafford Warren? What was his background that made him the logical one for that role?

Scott: Well, he was a radiologist in the first place, a very early-day radiologist, who was a colonel in the Army. He was assigned to the Manhattan District. When he was at Bikini he stayed on the same ship they gave me quarters in, the *Haven*. He was literally my boss.

Hughes: Did Hamilton have any role in the Bikini test?

Scott: Yes, he was flying back and forth. We saw him, I think, once or twice. Once he brought us a bottle of bourbon. The only bad thing about Bikini was there was no potable liquor. We used to drink laboratory alcohol and lemon-juice powder [from] a K ration. This beefed our drinking up to a potion which was 7 percent bourbon and the rest laboratory alcohol. We'd take a couple of slugs of that and then try to make it up the stairs where the general mess was. We ate in an officers' mess there.

Hughes: Why was Hamilton flying back and forth?



Scott: I really don't know. Something important, you know. Got to discuss something with somebody.

**Joseph Hamilton**

Hughes: What was your relationship with Hamilton like? Were you ever close to the man?

Scott: Well, I loved him as a brother, but was never too close. I used to spend some of my vacation time with him; he had a little cabin up in Downeville.

Hughes: Was he a person with whom one could get close?

Scott: No, I could never get very close to him. He was the kind of a guy who gets his zipper stuck in the men's room, he won't let anybody help him. I caught him in the men's room one day, and he couldn't get his zipper up, and he wouldn't let me help him with it. So I got him a pair of scissors. I guess he cut himself out of there one way or another. But he was very shy; only son of a very well-known neurologist or psychiatrist. He came from Santa Barbara.

Hughes: And they had no children either.

Scott: No, they didn't. The closest relation is a fellow.... Mrs. Hamilton had a sister who married one of the heads of the biochemistry department [at UCSF], Dr. [Frank Worthington] Allen. Allen was a peculiar duck. He died of cancer of the liver.

Hughes: Do you think that Hamilton's shyness influenced the way he ran Crocker Lab? Do you think it was a more isolated group because of the personality of its director?



Scott: I don't think he did anything other than anyone else would have done. We were under very heavy security all the time.

Hughes: How long did that last?

Scott: Right up until the day I left there, '51. Everything we did and published was classified. That place is full of declassified documents over there.

Hughes: So even if he'd wanted to have a closer scientific relationship with Donner Lab—and that was what was in the back of my mind when I asked that question—it would have been very difficult, just because of the secret nature of the work.

Scott: Yes, it could have been.

Hughes: But then on top of that you had the personal problems between Hamilton and John Lawrence.

Scott: That's right. I don't think they really had a problem; they were just competitors.

Hughes: Do you think that problem died down once Donner Lab opened and John Lawrence had his own kingdom, so to speak?

Scott: That's right.

Hughes: I noticed that Hamilton was made director [of Crocker Laboratory] as late as 1948. But he had been effectively director since the opening of Donner Lab.



Scott: In the very beginning he was running it, yes.

Hughes: What was the significance of that title, do you suppose?

Scott: Just a university.... They were backward. When I went to work for him again, he was an assistant professor in radiology and neurology, I think. And in spite of all the terrific things he did, it was very late in life that he was promoted to a full professorship.

Hughes: Why do you think that was?

Scott: The university was very reluctant to promote anybody.

Hughes: So it really didn't have anything to do, for example, with the problems of the medical school.

Scott: No. The medical school got the last end of it, in a way, I think.

Hughes: What do you mean by that?

Scott: Robert Sproul kept his thumb on everything, really. When they hired me in the radiology department, they hired me as an assistant professor with the understanding that I would be promoted to associate professor in a year. Well, that went on and on, and I finally found out because Robert Sproul stated it. He had it in mind to promote me, but I had to wait yet another year.

Hughes: Why was it so slow?

Scott: Most of it was budget. I think they just didn't have the money to pay people.



When I moved into radiology [at the medical school in San Francisco], before there was a Radioactivity Center, my annual salary was \$4,200 a year, and that was it. After Bikini I was turning down jobs for \$25,000 a year. One of them was Hunter's Point. I designed the building, [which] they've now abandoned or blown up or something, for decontaminating all those lousy Bikini-infested ships.

Hughes: Was that the sole purpose for the lab [there]?

Scott: Oh, that was their first problem when this was the end of the thing. The Navy offered me the directorship of that laboratory.

Hughes: I was thinking back to when you did leave for San Francisco in 1951. It seem to me that Crocker Lab was in a rather weak position. Hamilton was dead. Durbin didn't get a Ph.D. until 1953. Who was left to administer Crocker Lab?

Scott: Well, I guess nobody. They certainly didn't come around to me and ask for help. So I don't know. Hardin Jones was in there pretty heavy at that time. John Lawrence would have been.

Hughes: Why was Hardin Jones involved?

Scott: Well, he was kind of John Lawrence's right-hand man.

Hughes: He didn't have any research connection with Crocker, did he?

Scott: No, he liked to fiddle with my research data and turn it over to John as something that we'd done together, or some such thing. I gave him hell for that one time and



said I was very offended.

Hughes: Did you ever have any part in his work on the effects of low-level radiation? That became quite a controversy later on, when he was maintaining that the current acceptable standards of radiation were set way too high.

Scott: Well, he was probably right, but I didn't get into that.

Hughes: I think that might have been after you had moved to San Francisco. In fact I'm quite sure it was.

Scott: Yes, could have been.

Hughes: But you had virtually no dealings in the scientific sense with Donner Lab from its inception all the way up to the time you left, did you?

Scott: None.

Hughes: Why was the 60-inch cyclotron dismantled and sent to Davis?

Scott: Well, they needed a building for it, and UCLA got the 37-inch one. I guess Davis was next in line.

Hughes: And the building was destroyed, as you said before, simply because people—who was it? the university administration?—felt that it was contaminated?

Scott: Could have been the health physics department.

Hughes: Raymond Birge, the father, has written a history of the physics department, and there's a section in there on the



Division of Medical Physics. He maintains that one of the real problems with the medical physicists was the fact that anytime they were up for promotion—this is people like John Lawrence and Hamilton, I guess, the physicians, the M.D.'s—they would be blocked by people at the medical school on the faculty, I guess for the reason that you were referring to—thinking that they were not clinically adept.

Scott: Well, they'd accepted me, body and soul. I did nothing but work with clinicians for years, which I found most stimulating, because they have questions which are unusual and sometimes I could help them out.

Hughes: Why do you think that they accepted you, and were seemingly reluctant to accept John Lawrence and also Hamilton in the sense that Hamilton was slow to be promoted?

Scott: Well, a lot of them didn't know Joe Hamilton. It was a job where somebody had to get in there and get to know all these people personally and discuss their research problems with them and help them if they could.

### **Radioactivity Research Center**

Hughes: Why didn't Hamilton do that?

Scott: Well, he didn't have any facilities. He was on the committee which formally set up my laboratory. He used to come around once in a while, when he wasn't feeling too well, and remind me of the fact that the committee he was on was my advisory committee. Well, I didn't ask him anything. I knew what I wanted to do and I went ahead and did it.



- Hughes: Why do you suppose you were chosen for that position?
- Scott: I have no idea, except maybe for the university's experience with me at Bikini.
- Hughes: Could it also be something to do with the fact that you seemed to be able to get along with the clinicians and you were on good enough terms with Hamilton? In other words, you weren't a terribly controversial figure.
- Scott: Well, they didn't know then when they put me in that position. They knew I was one of Stone's protégés, which was something that was so much in my favor around there that I don't think anyone would have crossed him on it.
- Hughes: How did you become a protégé of Stone's?
- Scott: Through the Manhattan District, and before that.
- Hughes: What had been your relationship in the war years? How had you had dealings with him?
- Scott: I used to see him about once a month. I'd go back to the University of Chicago and have a dinner with him and talk to him.
- Hughes: About what you'd been doing?
- Scott: About what we were doing. The importance of it.
- Hughes: Do you think that he would shuttle projects to Crocker as a result of these conversations?
- Scott: Well, he was on many AEC advisory committees for health



and medicine; I'm sure he could have blocked any of them.

Hughes: Well, on the other side of the coin, do you think because you were on good terms with him, he was diverting projects to you and Crocker Lab? Or was that just the logical place for things to go?

Scott: [It was] just a way to do it; just get somebody who had the staff that could do it.

Hughes: What sort of an administrator was Stone?

Scott: Well, he was an excellent administrator, as you can tell from this little tape [transcript of a tape that Dr. Stone recorded in 1964 about the history of the Department of Radiology at UCSF]. Most of the medical specialties are highly competitive where people are in the same area, like the surgeon and the bronchologist and the urologist, and the surgery department was another case in point. Or it could be neurology too.

Joe Hamilton got himself involved in developing the unit that I directed for so many years, the Radioactivity Research Center. A decision was made on the San Francisco campus as to what it should be. Stone and Low-Beer were very heavily involved in it, and claimed they started it. I hadn't been there very long when I was walking down the hall one day with Dr. Stone, and the dean, Dr. [Francis] Smyth, came out, and offered me the directorship of the Radioactivity Center right then and there. I was naturally very happy about it. This was after my Ph.D, by the way, and made Stone furious. He said he was going to see that I never got promoted, or do another thing for me. I said, "Do you want me to resign?" And he said no.



- Hughes: Why was he furious?
- Scott: Because he wanted to control it. He wanted to run it.
- Hughes: But you were at that stage in the Department of Radiology. Wasn't the Radioactivity Research Center a branch [of the department]?
- Scott: No, it was set up on its own budget.
- Hughes: So he really didn't have much control.
- Scott: Didn't have any. So they were sniping at me for years over that, as long as they were around.
- Hughes: What was Smyth's reason in appointing you director?
- Scott: Well, he spoke to the deans of pharmacy and dentistry, and that's what they wanted. They wanted somebody that could lead them around and help them out and not control it like a patent.
- Hughes: What happened after the initiation of the Radioactivity Research Center and the fact that [Stone] was upset that he didn't have control over it? What did that do to your relationship with Stone?
- Scott: Well, it degraded into an unfortunate situation, unfortunate for me. I had other research facilities on the campus. I got some cancer research funds and I furnished a little building above the unit which had facilities for the ophthalmologists. I found the money for the furniture and a lot of the laboratory equipment. Their building, their walls were put up by a grant from radiology; they had various funds.



Hughes: Did the coolness with Stone have any effect on what you were trying to do?

Scott: Well, I had to work through him and to get cancer money and eventually the grants dried up and the American Cancer Society and the Cancer Research Institute, which was another entity on the medical campus, were very upset with me when I began curing animal tumors with LSD. They thought that was just poor judgment on my part and they dropped all support finally.

Hughes: Why "poor judgment"? Because it could be used for other purposes?

Scott: Well, they thought [of] LSD and its connotations with the social scheme of things, and all of the difficulties that people got into with acid and so forth.

Hughes: It wasn't enough that it was curing cancer?

Scott: Didn't seem to faze them at all. The American Cancer Society is a very peculiar group. They don't want any new discovery. I'd worked as much on cancer as I did on anything else. I came up with a cancer test involving red-cell kinetics with rubidium, where we got very highly significant results statistically. I went to the World Health Organization in Geneva and everywhere else I could go. And I never got it off the ground.

Hughes: Why was that?

Scott: Well, people would be interested, but then they'd say, "We'll let you know." It fell flat on its face.

Hughes: What was the whole purpose of establishing the



Radioactivity Research Center?

Scott: It was to give the campus research potential in whatever specialty [it was] interested in.

Hughes: So this was supposed to be an institution that cut across departmental lines?

Scott: Right.

Hughes: How did that work?

Scott: It worked for me very well, and I think it worked for the recipients very well. We did a lot of things for a lot of people, some of whom don't even remember it. Among the people that have become quite famous at the medical center, I got a lot of those people started. One of them was [Isidore S.] Edelman, who is a space-scientist type of person. I got Dr. [Richard J.] Havel started with just enough space to keep a dog, and helped him out with a few counts now and then. He's now, I guess, director of the Cardiovascular Research Institute.

There was another fellow by the name of Don Pickering, who was interested in monkeys, and he was breeding monkeys. It was rather rare in those early days to successfully breed monkeys and raise the offspring and start a colony that way. He was just like breeding a monster. His demands for space got greater and greater and we were shrinking and shrinking and finally he went to the University of Oregon [at Portland] with his colony and all. He was the kind of person that had trouble dealing with people. He couldn't share anything. So they finally threw him out up there in some big political push. And that was the end of Donald Pickering.



- Hughes: The Center, then, was set up to provide research space?
- Scott: What I did for all of those people was perfectly logical and normal in our operating area. We were there to help anybody that had a problem that we could help [with].
- Hughes: They would come to the Center to do their research and you would supply the radioisotopes?
- Scott: They'd come to me and I'd have a discussion with them and decide if what they wanted to do was appropriate or not, or if we could fix it so it would be appropriate. I could loan them the equipment or I could loan them the facilities. I had a technician and kind of a physicist as part of my staff. We would hand-feed them along with this until they either became successful or satisfied their curiosity.
- Hughes: And then they'd move out and somebody else would move in?
- Scott: Yes.
- Hughes: What was producing the radioisotopes?
- Scott: We were buying most of those. Some of them came from the 60-inch cyclotron.
- Hughes: You never had a cyclotron on that side of the Bay?
- Scott: No.
- Hughes: Didn't Stone have a synchrotron?
- Scott: Yes.



Hughes: Was that strictly for therapy?

Scott: Well, it was. There's another case in point where a group like the Atomic Energy [Commission] sets up a unit—I don't care whether it's here or Harvard or anywhere else—and these people move in and the first thing they do is shut the door and you never get in there again. They go on their own little projects.

The synchrotron was like that. It was run as Stone used to run the Metallurgical Laboratory at the University of Chicago. They had a Q-clearance and so forth.

Hughes: This was in the Fifties, wasn't it?

Scott: Well, it started out earlier than that, yes.

Hughes: Why would you need a Q-clearance for therapy?

Scott: Don't ask me. Nothing secret in there except this great big bloody machine they could never get to run right. And when they finally got it going real good the physicist [Gail D. Adams] that was running it divorced his wife and left for the University of Oklahoma. And I can't get him to answer letters or anything.

Hughes: Was it a human problem or a mechanical problem?

Scott: His problem was mostly mechanical. He was a very capable guy, and he and I were trying to put some of my plots on a computer so we could get a computer read-out of the data and so forth.

Hughes: Was Stone doing any therapy on the machine at that stage?



Scott: He took it over after they really got it running. He had a Filipino physician—I don't remember her name—who treated quite a few patients under his direction.

Hughes: How successful was that?

Scott: Well, you'd have to take Stone's word. He said it was good. But when he retired nobody would give him any patients. See how the worm turns?

Hughes: Well, it sounds, the way you describe it, like a pretty dog-eat-dog situation over there.

Scott: That's what academic life is.

Hughes: You think it's across the board like this, or do you think this particular milieu is worse than others?

Scott: I don't think it's any different. I think everybody's like that in any kind of competition. I took on 17 Ph.D.'s that the University of Chicago hired for their Argonne National Laboratory, and I skunked them every time. That was because they just didn't know what they were doing. And that's why they won't speak to me any more.

Hughes: You mean "skunked them" in a scientific sense? You proved them wrong scientifically?

Scott: In publishable results. Some of our findings I guess they'll never resolve. I found a different distribution and certainly a different risk to plutonium that they couldn't repeat.

Hughes: Have you ever thought of going back to your pre-war research when you were concerned more with everyday



physiology rather than fission-product metabolism, the bomb-related work?

Scott: Well, I really did in my cancer research when we used rubidium and red cells. We were only getting our red cells from cancer patients. Then I got some data from blood samples that came from the members of the Department of Radiology. They fell into a different group statistically than our normals. I called it radiation exposure. And that's where this test came from. That test is just straight physiology and nothing else.

[End of interview]



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**Curriculum Vitae**  
**Sally Smith Hughes**

1963	B.A. (anatomy), University of California, Berkeley
1966	M.A. (anatomy), University of California, San Francisco
1972	Ph.D. (history of medicine), Royal Postgraduate Medical School, University of London
1966–1969	Postgraduate Research Histologist, Cardiovascular Research Institute, University of California, San Francisco
1978–1980	Historian, Medical Physics Project, History of Science and Technology Program, The Bancroft Library, University of California, Berkeley
1980–	Research Associate, Department of the History and Philosophy of Health Sciences, University of California, San Francisco
1980–	Research Associate, Office for History of Science and Technology, University of California, Berkeley
1980–	Interviewer, History of Science and Technology Program and Regional Oral History Office, The Bancroft Library, University of California, Berkeley
1980–	Recipient, National Institutes of Health grant for a history of nuclear medicine

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